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## ***Interactive comment on “Effects of N and P fertilization on the greenhouse gas exchange in two nutrient-poor peatlands” by M. Lund et al.***

**M. Lund et al.**

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*We would like to thank the reviewer for careful reading and constructive comments on the manuscript. Our responses to the comments can be found in italics below.*

General comments: The manuscript presents results from fertilization experiments where short term effects of added N and P on exchange of CO<sub>2</sub>, CH<sub>4</sub> and N<sub>2</sub>O have been measured. The southern study site, a temperate bog, Fåjemyr, has sevenfold N deposition in comparison to that of the northern (artic) bog, Storflaket. The layout and findings of the present study are not very novel or unexpected. However, it is a nice work and worth of publishing in Biogeosciences. Presentation of the results is not always clear (e.g. comparisons). Please, check the manuscript, especially the results-section, thoroughly for language, clarity and structure.

C1534

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Discussion Paper



Specific comments: Title: Could “. . .in two nutrient-poor peatlands” replaced with “... in two boreal peatlands with contrasting N deposition rates”? Then it would better raise up the differences between the two sites (which can also be seen in the results).

*Title changed to “Effects of N and P fertilization on the greenhouse gas exchange in two northern peatlands with contrasting N deposition rates”*

Introduction p. 4806 row 5 and Discussion p. 4817 row 23: “Methanogenic bacteria “ should be “methanogenic archaea “ (in the past they were called archaeobacteria, but due to their independent evolutionary history and biochemical differences from other forms of life they are now classified as a separate domain in the three-domain system (Archaea, Eukarya, Bacteria). Introduction p. 4806 rows 12-16: Addition of nitrate can also inhibit methanogenesis e.g. by competition for hydrogen and or due to toxicity of denitrification products (see reviews by R. Conrad 1999 FEMS Microbiology 28, 193-202 and R. Conrad 2007 Advances in Agronomy 96, 261-323 and references in those).

*Changed according to reviewer suggestion.*

Introduction p 4806 row 25: Please use term “net ecosystem exchange” (defined earlier) instead of “net primary production”

*In this particular study, NEE was not studied. No action taken.*

Introduction p 4807 row 2: I would replace “all greenhouse gases” with “three greenhouse gases” because CO<sub>2</sub>, CH<sub>4</sub> and N<sub>2</sub>O are not the only (although are the most important) greenhouse gases. Introduction p 4807 rows 8-10. Were the hypotheses written before the study? They happen to be exactly the same than the received results. . .In the last paragraph I would rather point out the aim and originality of this study in comparison to existing knowledge e.g. from previous fertilization experiments conducted in boreal peatlands.

*Changed according to reviewer suggestion.*

Materials and methods p.4808 chapter 2.3: Were fluxes measured before the fertiliza-

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Discussion Paper



tion experiments to see if control plots and treated plots were originally similar?

*We only have one day of measurements, from which no general differences can be found. However, we found that spatial variation in CO<sub>2</sub> was explained by the amount of shrubs (and sedges explained spatial variation in CH<sub>4</sub>), and this was accounted for in the analyses. No action taken.*

Materials and methods p.4808 row 24: Were the GHG fluxes in Storflaket measured in 2007?

*Yes, this has been clarified in the manuscript.*

Materials and methods p.4809 row 2: Replace “during” with “before” GHG flux measurements

*Changed according to reviewer suggestion.*

Materials and methods p.4810 rows 17-19: How many headspace gas samples were taken during the measuring period? What is “significant” flux?

*Three headspace gas samples were taken for the gas chromatography analyses. The Innova takes six samples during its measurements. Significant flux refers to  $r^2$  higher than 0.7, this has been clarified in the manuscript.*

Materials and methods p.4810 row 11: Requirement of  $r^2$  value of 0.7 or higher can be problematic for N<sub>2</sub>O fluxes. For small fluxes  $r^2$  value is small, although the measurement has been successful. Thus, in the case of N<sub>2</sub>O fluxes,  $r^2$  limit of 0.7 can lead to a significant data reduction.

*Yes, it is an important problem that small fluxes will have small  $r^2$  values. Fluxes of both N<sub>2</sub>O and CH<sub>4</sub> can also have high temporal variation which can cause nonlinear time series. However, the Innova is a multigas analyzer and the temporal resolution is thus low. Using an  $r^2$  threshold of 0.7 assures that the measurements are reliable, which can be illustrated by the close agreement with gas chromatography. No action*

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Interactive Discussion

Discussion Paper



taken.

Materials and methods p.4810 row 16: Was the data always normally distributed within each test group? Was LN or some other transformation used to achieve the normality?

*Yes, data was always normally distributed according to Kolmogorov-Smirnov test. No action taken.*

Materials and methods p.4811 rows 13-14: ANOVA was also used for N<sub>2</sub>O fluxes (p.4812 row 25)? What about CH<sub>4</sub>?

*This section refers to the time series modeling of GPP and Reco in Fäjämyr. For CH<sub>4</sub> fluxes, RM-ANOVA was used. As the amount of data was low for N<sub>2</sub>O, only an ANOVA using annual averages were performed. No action taken.*

The Results section is not always clear, e.g. p. 4812 row 12-13: increase in what? Page 4812 row 13: “Respiration fluxes” could be replaced with “respiration”. Page 4812 rows 15-16: higher THAN (check the language).

*Changed according to reviewer suggestion.*

Results p.4812 row 1: Has PFT been determined somewhere?

*PFT has now been defined in Materials and methods section 2.4.*

Results p.4812 row 21: Where (what treatment) and when were the mentioned N<sub>2</sub>O peaks measured? Do you have any explanation for those peaks?

*This has been clarified in results section and discussed in discussions section.*

Results p.4813 row 6: Does “CO<sub>2</sub> uptake” mean here both NEE and GPP?

*It referred only to GPP, this has been clarified.*

Results p.4813 row 11: “There was a close to significant treatment effect for CH<sub>4</sub> fluxes. . . .” could be formulated more clearly, e.g. “Nitrogen and phosphorus alone slightly increased CH<sub>4</sub> emissions, while their combination decreased CH<sub>4</sub> emissions”.

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Interactive Discussion

Discussion Paper



*Changed according to reviewer suggestion.*

Results p.4813 row 18: Were both sites always net sources of CH<sub>4</sub>? If yes, “CH<sub>4</sub> emission” would be more informative than “CH<sub>4</sub> fluxes” (term “flux” does not tell the direction and is thus less informative). Methane emissions were surprisingly low, discuss the possible reasons.

*CH<sub>4</sub> fluxes changed to CH<sub>4</sub> emissions according to reviewer suggestion. Possible explanations for low methane emissions are now included in discussions section.*

Results p. 4814 rows 7-8: I don't understand what does “average sums” mean here, averages? Are these values for Fäjemyr? Does the data presented in Fig 3 (positive NEE value-> C source) include all seasons? That would explain the positive values in Fig 3, although in Table 2 (spring, summer and autumn) NEE is always negative (C sink).

*The section has been clarified. The numbers refer to the modeled time series of GPP, Reco and NEE in each plot in Fäjemyr. This data include seasonal and diurnal dynamics between March and November, while the values presented in Table 2 are instantaneous daytime rates.*

Results p.4813 row 23: Temperature and respiration were always correlating. Was this taken account in the measurements (e.g. by randomizing the measurement order of the plots)?

*Yes, the measurement order was always randomized to minimize the effect of temperature and other factors. No action taken.*

Tables 1 and 2: The legends could be more informative, e.g. Fig.1 Effect of N and P fertilization treatment on CO<sub>2</sub> component fluxes. . .and Fig 2. Seasonal averages for CO<sub>2</sub> component fluxes. . .In table 2, I would prefer means and standard deviations instead of estimated marginal means (no p- values presented, thus RM-ANOVA is not needed here). Please, explain abbreviations and meaning of positive/negative sign as

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6, C1534–C1540, 2009

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Interactive Discussion

Discussion Paper



a footnote. Could table 1 and 2 to be merged?

*Tables 1 and 2 have been merged according to reviewer suggestion. However, we choose to keep the estimated marginal means as these numbers account for the cover of sedges and shrubs which was found to impact the CH<sub>4</sub> and CO<sub>2</sub> data, respectively. This also accounts for Fig 3 where the CO<sub>2</sub> component data has been detrended for the spatial variation in shrubs cover. This adjustment is important to account for non-treatment effects. Abbreviations and positive/negative sign have been explained.*

Figure 1: Is this necessary? This map can be derived from the internet if needed.

*It is true that similar maps can be obtained from reports on the internet. However, this map is created from data obtained from SMHI and we believe that it provides a nice illustration of the spatial variation in N deposition in Sweden.*

Figures 1-3: Could the symbols/bars be made more clearly visible in black-and-white printout.

*Changed according to reviewer suggestion.*

Figure 2. It is said here (not in materials and methods) that nutrients were added just before the GHG flux measurements were conducted on day 1. Can the peak on respiration during the first day (Storflaket) result from increased soil moisture content due to the irrigation? To me it seems that respiration has been induced also in the control plots of Storflaket.

*It is said in section 2.3 (P4808 L25-26) that nutrients were added on day 1. Potential reasons for these peaks are discussed on P4815 second paragraph, including the irrigation effect. No action taken.*

Discussion p.4816 row 2: HAVE an additional Discussion p.4816 row 4: . . .to CO<sub>2</sub> in the aerobic zone OF PEAT PROFILE.

*Changed according to reviewer suggestion.*

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6, C1534–C1540, 2009

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Discussion Paper



Discussion p.4817 rows 24-25: “. . .strong possibility for increased N<sub>2</sub>O emissions with increased N availability. Is this conclusion justified, because the N<sub>2</sub>O fluxes were minor and there were no statistically significant differences between the treatments (p. 4812 rows 20-25)?

*We agree that this is an overstatement and have removed the wording “strong”.*

Conclusions p.4818 row 5: What do these complex and nonlinear responses in CH<sub>4</sub> exchange mean?

*This has been changed to “No significant effects of nutrient addition were found for the CH<sub>4</sub> exchange”.*

Reference list: When referring to Persson et al 2004 it is probably better to refer to [www.smhi.se](http://www.smhi.se) where the mentioned pdf can be found. The given internet address did not work.

*Changed according to reviewer suggestion.*

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Interactive comment on Biogeosciences Discuss., 6, 4803, 2009.

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6, C1534–C1540, 2009

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